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JOURNAL

OF THE

MASSACHUSETTS ASSOCIATION

OF

BOARDS OF HEALTH

An Official Quarterly Record of
Information for the Public

MARCH, 1899

Proceedings of the January Annual Meeting

Subjects: Carbon Dioxide Determination — Branching Form of Bacillus Diphtheriæ — Rabies in Boston — The Refuse Disposal Plant of Boston

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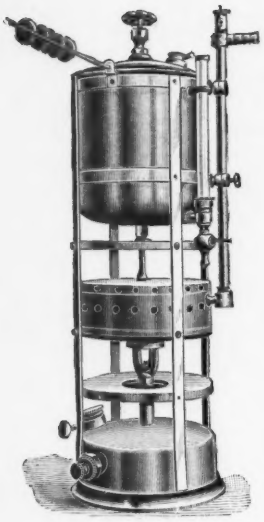

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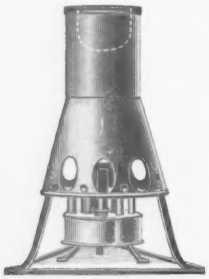

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THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH was organized in Boston in March, 1890, with the following objects: the advancement of sanitary science in the Commonwealth of Massachusetts; the promotion of better organization and co-operation in the local Boards of Health; the uniform enforcement of sanitary laws and regulations; and the establishment of pleasant social relations among the members of the Association.

All persons holding appointments as members of a Board of Health in a Massachusetts city or town, the executive officers of such a local board, and the members of the State Board of Health are eligible to membership. Other persons may be elected members by vote of the Association. The annual dues are one dollar and fifty cents, and should be paid to the Treasurer, James B. Field, M.D., 329 Westford Street, Lowell, Mass.

The Association holds four regular meetings each year, the annual or January meeting always being held in Boston.

THE OFFICIAL JOURNAL OF THE ASSOCIATION is a quarterly publication, containing the papers read at the meetings, together with verbatim reports of the discussions following them. No part of this matter is printed in any other periodical.

The JOURNAL will present, from quarter to quarter, a fair and adequate picture of the progress of practical sanitary science as applied to the needs of a modern community. The various subjects which are reviewed in the quarterly meetings of the Association are treated by experts qualified to speak from daily experience in Public Health offices, who, as men of science, are careful to be scientific and comprehensive, and who, as public officers, are no less careful to speak pertinently and so as to be easily intelligible to the layman.

The JOURNAL, in a word, appeals to all whose interests touch the questions of sanitation and hygiene,— to the architect, the school-committee-man, the manufacturer, the contractor, and, above all, to the busy practitioner who has no time for any reading but what is brief and to the point.

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All communications to the Association should be addressed to the Secretary, **EDWIN FARNHAM, M.D., City Hall, Cambridge, Mass.**

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JOURNAL OF THE

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ORGANIZED 1890.

[The Association as a body is not responsible for statements or opinions of any of its members.]

VOL. IX. March, 1899. No. 1.

JANUARY MEETING

OF THE

Massachusetts Association of Boards of Health.

THE January quarterly, or regular annual meeting, of the Massachusetts Association of Boards of Health was held in Boston, at the Parker House, on the afternoon of Thursday, January 26, Dr. Samuel H. Durgin, vice-President, in the chair.

THE CHAIRMAN.— If any one has a name to be submitted for membership in the Association, he should write it upon a card, and give it to a member of the Executive Committee. This committee will meet now at this end of the room to receive and act upon those names and any other business which may come before it. Names should be handed in at once.

HON. E. L. PILSBURY.— Might it not be a saving of time to appoint a committee to nominate officers while the Executive Committee is in session?

THE CHAIRMAN.— Very good. We should come to that in a few minutes, but perhaps it would be well to have the committee appointed at once.

HON. E. L. PILSBURY.— I move, Mr. Chairman, that a committee of three be appointed by the chair to serve in that capacity.

The motion was adopted.

THE CHAIRMAN.— The chair will appoint Mr. Coffey, of Worcester, Dr. Gage, of Lowell, Mr. Pilsbury, of Boston.

After the meeting of the Executive Committee, the meeting of the Association was resumed.

THE CHAIRMAN.— While the committee is out, and the Secretary also out at the telephone, we shall have to wait their return before we can proceed with the programme. We can, however, listen to the report of the Treasurer, Dr. J. B. Field, of Lowell.

Dr. Field then presented the following report: —

TREASURER'S REPORT FOR 1898.

RECEIPTS.

Balance from 1897	$573.73
Interest at savings-bank for two years	37.46
Annual assessments	219.00
	$830.19

EXPENDITURES.

Postage and revenue stamps	$34.90
Printing	28.65
Cigars and dinner for guests	11.50
Clerical assistance	3.50
Total expenditures	$78.55
Balance to 1899	751.64
	$830.19

Of this balance of $751.64 to next year, $537.46 is drawing interest.

Respectfully submitted,

JAMES B. FIELD, *Treasurer.*

Examined and approved as correctly cast and properly vouched.

J. ARTHUR GAGE, *Auditor.*

The report was accepted, and placed on record.

THE CHAIRMAN.— The next business is the reading of the records of the last meeting by the Secretary.

Dr. Farnham read the records of the meeting which was held at Lawrence, Oct. 27, 1898.

THE CHAIRMAN. — Are there any corrections to be made in the records? If not, they will stand as approved. The Secretary will read the report of the Executive Committee.

Dr. Farnham read the names of the following gentlemen, all of whom were elected members of the Association upon his motion: —

THOMAS HUNT	Cambridge.
RUSSELL S. NYE	Falmouth.
FRANCIS P. DENNY, M.D.	Brookline.
P. F. LEARY, M.D.	Turner's Falls.
EMIL C. STIEGLER	Lawrence.
JOHN GILBERT	Fall River.
HORACE E. MARION, M.D.	Brighton.
DAVID W. TINSLEY	Fitchburg.

THE CHAIRMAN. — There has been a vacancy in the committee for the supply of scientific papers by the resignation of Dr. McCollom, of Boston. The filling of this vacancy rests with the Executive Committee. That committee has appointed Dr. Curtis of Newton to fill the vacancy. We will next listen to the report of the Committee on Nominations.

HON. E. L. PILSBURY. — I will read this list of nominations, because the extreme modesty of my colleagues, who are both candidates for office, forbids their presenting the names: —

President.

HENRY P. WALCOTT, M.D., of Cambridge.

Vice-Presidents.

SAMUEL H. DURGIN, M.D., of Boston. S. W. ABBOTT, M.D., of Wakefield.

Secretary.

EDWIN FARNHAM, M.D., of Cambridge.

Treasurer.

JAMES B. FIELD, M.D., of Lowell.

Executive Committee (for two years).

J. C. COFFEY, Esq., of Worcester. W. H. CHAPIN, M.D., of Springfield.
H. L. CHASE, M.D., of Brookline. J. A. GAGE, M.D., of Lowell.
R. L. NEWCOMB, Esq., of Salem.

And there is a vacancy in the committee list which will expire in 1900, by the resignation of Nathaniel Hathaway, of New Bedford; and the name submitted is that of Thomas W. Cook, of New Bedford.

Respectfully submitted by the Committee.

THE CHAIRMAN.— Gentlemen, you hear the report of the committee. What is your pleasure?

COLONEL MORSE.— I move that the Secretary be authorized to cast one ballot for the election of the officers named by the Nominating Committee.

The motion of Colonel Morse was adopted.

DR. FARNHAM.— Mr. President, I hereby cast this ballot for officers of the Association.

THE CHAIRMAN.— The officers named have been elected. Is there any miscellaneous business to come before the meeting? If not, we will proceed to the first paper on the programme, "A Ready and Accurate Method of Carbon Dioxide Determination, with Demonstration."

A SIMPLE AND ACCURATE METHOD OF CARBON DIOXIDE DETERMINATION, WITH DEMONSTRATION.

BY G. W. FITZ, M.D., OF HARVARD UNIVERSITY.

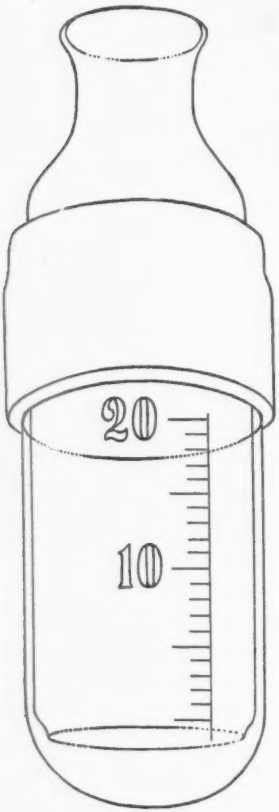
Quantitative methods for the determination of carbon dioxide have long been sought,— formerly, because it was supposed to be the dangerous part of respired air; at the present time, because the amount produced by the processes of vital oxidation is thought to be directly proportional to the quantity of the volatile poisonous elements. The methods in use for its determination have been many. Pettenkofer's method, with recent modifications, is accepted as the most exact, except perhaps certain absorption methods that can only be carried out in a laboratory. The "ready" methods for its determination, however, have been fairly numerous.

Wolpert's method, used by the State police for the examination of school-houses, is one of the earliest. It depends, in principle, on the fact that lime-water becomes milky and opaque through the formation of calcium carbonate by union with the CO_2 of the air tested. A standard opacity is determined by noting when a black spot can no longer be seen through a certain depth of the milky solution. The method is quite inaccurate, varying with the rapidity with which the air is forced through the solution.

The younger Wolpert has devised a method, which is much more accurate, depending upon the neutralization of a standard alkaline solution (sodic carbonate), using phenolphthalein as the indicator. Wolpert has used this for testing the purity of air in contact with the body under the clothing, and for air in adjacent rooms as well as for the ordinary determinations. The method is accurate and convenient. The solution, however, must be made with care, since a varying amount of water of crystallization in the sodium carbonate makes serious variation in the strength of the solution. For ordinary school examinations the method is beyond the technical skill of the teachers, and hence unreliable.

In order to get a simpler method, I made some experiments with

lime-water, to determine how far a saturated solution would serve as a standard solution. I tested a number of saturated solutions from lime bought in different parts of the country, and found that the alkalinity of 100 c.c. was practically uniform in all. It was thus found possible to make a standard solution by merely measuring out a stated amount of a solution of lime-water saturated in the presence of an excess of the hydrate and at ordinary room-temperature.



After various experiments, I found that an apparatus such as I now present answered both for measuring the volume of the air used and for shaking the air and lime-water together. It is simply a cylinder or cup of glass, with rounded bottom, and a smaller open cylinder, with a neck resembling that of a bottle at one end. The smaller cylinder is made to slip into the larger through a collar of rubber tubing, so that it forms an air-tight piston. When the inner cylinder is drawn out, air enters through its mouth. The amount of air drawn in can be measured by means of the graduations on the side of the larger tube, the bottom of the inner tube serving as an index.

A 1 per cent. solution of a saturated solution of lime-water was found most serviceable for the tests, and may be made as follows: about 95 c.c.'s of water, to which several drops of the phenolphthalein solution have been added as an indicator, are neutralized by the addition, drop by drop, of lime-water with shaking. As long as the pink color caused by the lime-water

disappears upon shaking, the addition is to be continued; but, when a faint tinge of pink remains, we may consider that all the carbon dioxide in the water has been neutralized. By the addition of 1 c.c. of lime-water and enough water to fill the flask to the 100 c.c. mark, a standard 1 per cent. solution is formed, since none of the 1 c.c. of the lime-water is neutralized or lost. 10 c.c.'s of this solution are taken out by means of a 10 c.c. pipette, and put into a 3-dram homœopathic vial. This solution holds its strength for about twelve hours, and, when put into vials, can be carried anywhere in one's pocket. The whole operation of making up the solution requires five to ten minutes, and is exceedingly simple. The only apparatus necessary is a 1 c.c. pipette, a 10 c.c. pipette, and a 100 c.c. flask.

To make a CO_2 determination, the contents of one vial are poured into the testing apparatus. The inner cylinder is introduced, pushed down to the bottom, and then raised to the tare mark on the outer glass. This gives 30 c.c.'s of air for the first shaking. The right index finger is closed over the mouth, and it is shaken vigorously 30 times. If the color remains, the inner cylinder is run down to the bottom and drawn up to the 20 c.c. mark, giving 30 c.c.'s + 20 c.c.= 50 c.c.'s of air. This is repeated, and an account is kept of the amount of air used until the point of decolorization is reached. When great accuracy is required, the first test may be assumed to be experimental, and the test repeated with careful additions of small amounts of air toward the end.

As soon as the amount of air has been determined, the number of parts of CO_2 in 10,000 can be read off in the table, and the whole operation is complete.

TABLE.

Air in c.c. used.	*CO_2 in* 10,000.		*Air in c.c. used.*	*CO^2 in* 10,000.	
30	28	Very bad.	103	8	Fair.
36	22		117	7	
46	18				
			138	6	Good.
58	14	Bad.	165	5	
69	12		207	4	
82	10				
91	9				

When a large number of observations is to be made, it is simpler to take bottles holding 100 c.c.'s of the solution and a 10 c.c. pipette by means of which the amount necessary for each test can be transferred directly to the apparatus.

A comparison of the method with that of Pettenkofer shows that it is practically exact. I think I can say that, in the hands of an ordinarily careful man, it is exact within one part of CO_2 in 10,000 of air, which is a greater degree of accuracy than is required for ventilation determinations.

The testing solution will not keep its strength longer than one day. Various methods * for preserving the solution have been tried without success. If the bottles and stoppers are boiled, and sterilized water is used for making up the solution, it may retain its strength fairly well for two or three days. This is, however, so uncertain, and the solution is so readily made, that the question of preservation is practically unimportant. In ten minutes one can make up enough solution for a full day's tests.

I will pass the apparatus about.

THE CHAIRMAN.— Any remarks to be made upon Dr. Fitz's paper?

DR. SMITH.— I should like to ask Dr. Fitz what causes the deterioration of the solutions. Precipitation of the lime carbonate?

DR. FITZ.— It seems to me to be due to the evolution of CO_2 in the water and the consequent change of the hydrate of lime to the carbonate, doubtless through bacterial activity.

DR. SMITH.— Lime-water is antiseptic.

DR. FITZ.— Saturated lime-water will keep perfectly well, but this is only a 1 per cent. solution.

THE CHAIRMAN.— Professor Kinnicut, of Worcester.

PROFESSOR KINNICUT.— I can add very little to the paper that we have had the pleasure of hearing from Dr. Fitz. Certainly, we owe to him a very easy and very accurate method of determining carbon dioxide in the air. I have used Dr. Fitz's method in my own laboratory, in the Polytechnic Institute in Worcester, and have had

* Dr. M. H. Bailey made a large number of experiments with this in view, but found nothing really satisfactory.

my students use it during the last six months, and I find that analyses made by Dr. Fitz's method agreed very closely with analyses made by Dr. Pettenkofer's method, which, as Dr. Fitz says, is a method that can only be carried out by a man trained in analytical work and where he has a chemical laboratory at his disposal. The results of the two methods agree to within one part in ten thousand, and with careful work they agree even closer. For instance, during the past month one of the students of the Senior Class has been studying the ventilation of the Worcester Public Library; and I find in looking over his work the following results: —

One series of analyses gave 7.15 parts of carbon dioxide in 10,000 parts of air by Hesse's method, a modification of Pettenkofer's method, and 7.3 by Dr. Fitz's. Another series gave 12 parts by Hesse's and 12.8 by Fitz's. A third series, 6.3 parts by Hesse's and 7 by Fitz. These results show that Fitz's method gives a little higher result than that obtained by Hesse's, and I have found this to be the usual rule; but, the difference not being more than 1 part in 10,000, the variance is of small account, when studying the ventilation of rooms or halls. By using Dr. Fitz's method a large number of analyses can be made during the morning session of a school. One man can make 50 to 75 determinations in a morning, and the results compare most favorably with those obtained by Pettenkofer's or Hesse's method, where each determination takes at least one-half hour. Working in this way by Dr. Fitz's method, the effect of airing the room by opening the windows, and the sudden change that takes place in the air of the room during a recess, is very prettily shown. An interesting study can also be made with Dr. Fitz's apparatus of the effect of burning kerosene oil. I have often had my students take the apparatus to their rooms, and study the effect of a burning study-lamp on the air of the room. The amount of carbon dioxide in the air at the beginning of the evening being between 5 and 6 parts in 10,000, the student will often find that it reaches 22 to 25 parts at the end of the evening. This is not to be wondered at, as one student lamp will give off about 3 times as much CO_2 per hour as one person will exhale in that time. I suppose that Dr. Fitz will agree with me that really these 22 parts —

most of it coming from the lamp, and not from the breath — give no trouble or any disagreeable symptoms. In this connection there is one thing we should remember; and that is that carbon dioxide is not in itself, in any quantity that is found in a room, poisonous. In fact, work can be done easily in air that contains 1 per cent. of this gas. Carbon dioxide, as it occurs in the air of a room, is only a comparative measure of certain poisonous substances given off in the breath or from the person, whose exact nature we do not know, and is also only a measure of the impurities when the carbon dioxide comes from the breath, and not from lamps or gas.

I have also had my students using Dr. Fitz's apparatus determine roughly the amount of fresh air that enters a room. If the air of the street contains 5 parts of carbon dioxide in 10,000, and if each person breathes out 22 litres of this gas per hour, to keep the air of a room so that the carbon dioxide shall not be above 7 parts in 10,000, which is now required by the best sanitarians, 3,000 cubic feet of fresh air must enter the room per hour for each person present. Consequently, by counting the number of persons present and determining the carbon dioxide each hour, we can say how much fresh air is entering the room. If we find that the carbon dioxide is 8 parts at the end of an hour, only 2,000 cubic feet per person is entering the room each hour. If 10, only 1,200 cubic feet. In conclusion, I think we all feel we owe a debt of gratitude to Dr. Fitz for working out so carefully a process which cannot help being of great use in enforcing better ventilation in our public schools.

Dr. Abbott.— Mr. Chairman, I should like to say just a word about the practical importance of this matter. The Board of Health of Boston has made a wonderful advance in the medical inspection of schools; but it looks mainly upon one side,— that is, the infectious disease side,— while the subject of ventilation has been placed by law in charge of the district police. About a dozen years ago a letter came to the office of the State Board of Health from the School Committee of a small town, asking how they could determine the quality of the air of a school-room. In looking about, I found this description of Wolpert's instrument in one of his papers, and had this apparatus made. I would like to show just where its defects are.

When the district police came to me, and asked where they could get this apparatus, I told them by no means to rely upon it, since it gave only approximate results.

The defects are these: First, there is a rubber bulb; and every one knows that a rubber bulb goes to pieces in a short time, and becomes worthless. For instance, this one, which has been lying unused for several years, is so stiff that it is useless. Second, it is not accurate. One man will squeeze it very tight, perhaps with both thumbs, another one with only one thumb; and you do not eject all the contents of the bulb. Hence the sample of the air of the room is not an equable sample, under all circumstances, in quantity. Third, when it becomes old, there is an escape of air at the joint where the glass tube enters the bulb. I find this one allows half the air, at least, to go out at the joint instead of through the opening at the end of the tube. There are quite a number of just such defects as I have described. Here is another, in the tube containing the lime-water. You have to rely upon the vision. The observer must depend upon his eyesight to inform him when the little black mark at the bottom becomes sufficiently obscured so that he cannot see it. So there are quite a number of defects that account for its unreliability. It seems to me that anything that is more reliable deserves consideration.

Professor Rietschel, of Berlin, has done some excellent work in this line, and published some years ago a report of his examination of the schools of that city,* with diagrams showing just this point that Professor Kinnicut has illustrated; that is, the increase in the amount of carbonic acid, and then the sudden drop at recess, and then the increase again afterward, and so on. This certainly is a very useful instrument; and it seems to me that something of the kind, such as has been devised by Professor Fitz, ought to be in use in all our schools, and not only in our schools, but in all public buildings where people congregate in large numbers.

DR. DAVENPORT. — I was not in early enough to hear the beginning of Dr. Fitz's paper. I should like to inquire, therefore, if he pays any attention to the temperature of the air at the time of making the experiments. If he does not, how are his results comparable

* Lüftung und Heizung von Schulen, Berlin, 1886.

when taking air at the different temperatures between 32° and 70° Fahrenheit?

A MEMBER.— Does the temperature of the air affect the result?

DR. FITZ.— I found that the ordinary range of temperature in rooms made no difference whatever, and I have reason to believe from my experiments that any temperature above freezing does not vititate the results. But, of course, with considerable ranges of temperature an allowance must be made for the density of the air, as is done in Pettenkofer's and Hesse's methods.

DR. BROWN.— Mr. Chairman, I should like to ask by whom this apparatus is manufactured, and where it can be had, and how much it costs, and if with it there are printed directions that will enable the purchaser to make the tests or learn how to make them.

DR. FITZ.— Mr. Chairman, the apparatus is made by the Knott Apparatus Company, 16 Ashburton Place, near the State House, Boston; and complete printed directions go with it. It costs two dollars ($2.00), including the case and pipettes.

THE CHAIRMAN.— It is strikingly interesting to listen to the paper of Dr. Fitz, especially as he has brought forth a very practical as well as an exact means for determining the amount of carbon dioxide in the atmosphere. I cannot refrain from asking Dr. Fitz, however, if by means of this first-rate method he has found any condition in the schools of Cambridge yet which has awakened any interest on the part of the city authorities [laughter]. Are there any further remarks to be made upon Dr. Fitz's paper?

DR. GAGE.— I should like to ask if it is necessary for a chemist to prepare this solution every morning, or can each individual prepare it for his own use under the directions?

DR. FITZ.— Any one can make the solution. It requires no skill.

THE CHAIRMAN.— The next paper on the programme is one "On Branching Forms of Diphtheria Bacilli," by Dr. H. W. Hill, of Boston. Dr. Hill has been affected in the last twenty-four hours by the prevailing epidemic, and is not able to read his paper; and he has asked Dr. Brough, of Boston, to read it for him.

Dr. Brough then read Dr. Hill's paper "On Branching Forms of Diphtheria Bacilli."

THE BRANCHING FORM OF BACILLUS DIPHTHERIÆ AS AN AID IN BACTERIOLOGICAL DIAGNOSIS.*

BY HIBBERT WINSLOW HILL, DIRECTOR.

The published articles of nineteen or twenty investigators, dating from the year 1887 to the present time, have established the fact that the bacillus tuberculosis is often found to branch. The bacillus of diphtheria has also shown a similar peculiarity. It has long been held by bacteriologists that bacilli, properly so called, do not branch. To make this subject clear, it is necessary to recall to you five principal classes of bacteria,— the cocci, the bacilli, the spirilla, the cladothricheæ and the streptothricheæ. The cocci are spherical in shape. The other four classes consist of individuals all of which are more or less elongated, forming rods or threads. The bacilli consist of those rods which lie entirely in one plane. They may be bent or curved, but are never twisted spirally. The spirilla, however, occupy more than one plane; for they are not only bent or curved, but also twisted so that they appear like a spiral spring or a cork-screw. These two classes, the spirilla and the bacilli, do not show branching when they divide, the two new rods thus formed often remain lying end to end, united by some intercellular substance, and long chains may thus result. Amongst the cladothricheæ, which are also elongated thread-like forms, the distinguishing feature is the fact that, when division takes place, the two rods thus produced do not separate from each other, nor do they remain lying end to end. The two new rods remain united by a small amount of intercellular substance, it is true; but, instead of lying end to end, one rod grows at the point of division, pushes a little to one side and elongates past the other rod. The second rod then seems to be a branch of the first rod, which now appears as the main stem. It is nevertheless quite evident that the two rods are independent of each other. In the streptothricheæ, the last of these classes and the highest form of the bacteria, true branching occurs. A little protuberance pushes itself out from the side of a rod, and then elon-

* From the laboratory of the Boston Board of Health.

gates more or less until the two rods lying together present a sort of an irregular T. Sometimes one rod will give rise to two or more branches, or a branch may give rise to another branch of its own. In this way somewhat complicated figures may result.

The extreme irregularity of outline found in the rod-shaped organisms which are called the bacilli of diphtheria is one of the main features by which the bacteriologist recognizes them under the microscope. The diphtheria bacillus is distinguished from almost every other known bacillus by this very irregularity. All bacteriologists know that, when, toward the end of a diphtheria case, the cultures show a diphtheria bacillus fairly regular in shape, the difficulty in recognizing it at once increases. If the bacillus of diphtheria were regular in outline, we could have no such diphtheria diagnosis service as we have at present; for the diphtheria bacillus would under such circumstances resemble other bacilli so closely that no one could differentiate them microscopically. It will be seen, then, that the irregular shape of this bacillus is of great practical importance. The question naturally arises, Why is this bacillus almost the only one which presents these irregularities? Generally, bacteriologists have been satisfied to accept the fact without offering a satisfactory explanation. It cannot, however, be merely a provision of nature to enable boards of health to diagnose the disease. Without going into the matter more deeply at the present time, I have ventured to record my belief that the explanation why the diphtheria bacillus differs from all other bacilli lies in the fact that it is not a bacillus at all. This is a simple explanation, and yet an explanation which is somewhat in advance of generally accepted ideas. Nevertheless, as already stated, this belief has been growing for some time among those bacteriologists who have paid special attention to this subject.

If, then, this bacillus is removed from the class of bacilli and placed among the streptothricheæ, or cladothricheæ, what practical advantage is gained? Of course, scientifically, it is clear that it should be placed among, say, the streptothricheæ, if it belongs to that class, even if nothing else is gained than a greater accuracy in classification.

In addition to this, however, such a change in classification would

simplify matters somewhat. It is much more easy to understand the curious forms of this organism which are found under the microscope, if they are considered as isolated portions of a branching form,— that is to say, degenerated, broken-down parts of a larger and higher plant,— than to wrench our ideas of the typical bacillus to suit the peculiarities of this one species. To consider the diphtheria organism a bacillus forces us to all sorts of extremities in trying to account for its swollen ends, its peculiar granules, and the branching forms which it sometimes presents. To consider it a member of a higher group accounts for all these peculiarities at once without straining our imaginations or credulity.

There is, however, a third and more practical view of the situation. Whether it is agreed to call the diphtheria organism a streptothrix or not, the fact remains that branching forms of this organism may be found in considerable percentage of the cultures taken for the diphtheria diagnosis. This is my own experience, and, as I have found on inquiring, the experience of others who have done much of this work. It is true that hitherto little regard has been paid to the subject. Since I began watching for these branching bacilli, I have been struck by the help in diagnosis sometimes given in doubtful cases by finding in the preparation under the microscope one or more of the branching forms. Every bacteriologist who has examined cultures for diphtheria diagnosis knows that it is often possible to state definitely, after a very short examination, that a given preparation does show or does not show the presence of the diphtheria bacilli. Every bacteriologist knows also, however, that in some cases a very careful and laborious microscopic search will still leave him in a doubtful frame of mind as to whether or not the organism he finds is really the diphtheria bacillus or not. This is especially true in those cases referred to, where the diphtheria organism is more regular in shape than usual, and therefore resembles more closely other bacilli. In such cases the finding of a branched form of the proper size and staining reaction may be of some considerable help. If a branched form cannot be found in a given doubtful case, the bacteriologist is no worse off than he would be if he did not pay any attention to the existence of branched forms at all. If a branched form is found, however, he has one more point on which to base his

diagnosis. I do not wish to give the impression that every branching organism which may be found in a culture from the throat is necessarily a diphtheria bacillus. On the contrary, I wish to make very clear the fact that branching forms of another organism do also occur. This other organism will, however, give little trouble, for it consists of very long and very wide rods and threads, it stains very uniformly and presents no granules nor unstained spaces; while, on the other hand, the branching diphtheria bacillus is made up of sections each of which is by itself not decidedly larger than an ordinary unbranched bacillus, and each section shows generally unstained spaces, granules, and swollen ends also like those of an ordinary bacillus. In those cultures where the sections of a branched diphtheria bacillus are regular in outline, the smaller size will still permit its differentiation from this other and larger organism.

DR. BROUGH.— Dr. Hill has some illustrations of this organism on the door at the end of the room which he wishes to show.

Dr. Hill then described the illustrations briefly.

THE CHAIRMAN.— Any remarks to be made upon Dr. Hill's paper?

DR. CONNELL.— Mr. President, I enjoyed Dr. Hill's paper very much. I appreciate also the difficulty that he has expressed in making a positive diagnosis of these diphtheria bacilli. It seems to me particularly difficult in the course of convalescence after local applications of antiseptics have been made. At such times and under such circumstances I have found that the diphtheria bacilli undergo changes, and consequently the difficulty of determining their true nature increases. Then, too, as a result of this, the question of deciding whether or not the house can be relieved of quarantine is harder to decide. The difficulty that I have found about the branching forms, as he suggested, too, is that they are so few in number compared to other forms that you cannot say positively just what their relation to the single rods may be. His paper, I think, is helpful; and, if we could find them more abundantly in our cultures, the help would be more beneficial than it is now. At any rate, the subject is one that is extremely interesting, and, I think, will be helpful to us in making positive diagnoses.

THE CHAIRMAN.— Shall we hear from Professor Smith?

PROFESSOR SMITH.— Mr. Chairman, I wish simply to emphasize the statements just made; namely, that the subject is a very interesting one, and, while it may be at present of but very slight practical importance in making diagnoses, it may lead us after a time to determine the origin and source of the diphtheria bacillus,— in other words, to trace it back to growths which are like it, and which possibly may help us to know something about the origin of the disease itself. Of course, this is a purely academic question; but still I think it interests us all. The forms that are presented there resemble in many respects those of actinomycosis and certain forms of the tubercle bacillus. These and the diphtheria bacillus may be found to date back to a common ancestry before they were disease-producing germs at all. The question, in this form, is of great interest to the biologist.

THE CHAIRMAN.—Any other remarks upon Dr. Hill's paper? If not, we will proceed to the next paper on the programme, "Rabies in Boston," by Dr. Alexander Burr.

RABIES IN BOSTON.

Although rabies is a comparatively rare disease, it presents such distressing symptoms when it does occur that universal attention is at once directed to it. The recent occurrence of several cases in Boston makes the subject of more or less interest at this time. Rabies is an acute febrile disease, having a varied period of incubation. It is found in all warm-blooded animals, but is more especially known to us among dogs, occasionally being found in horses, cattle, and other domestic animals. It is a well-recognized fact now that rabies is caused by the inoculation, directly or indirectly, of the virus from animals affected with the disease. Directly, as when a person or animal is bitten by a rabid animal, the virus being carried through the skin by the teeth of the rabid animal. Indirectly, as in the case of an animal licking a sore on an animal in the early stages

of the rabies; licking of posts, etc., which have become smeared with the saliva from a rabid animal; also the occupation by animals of kennels that have been previously occupied by rabid animals.

Man becomes infected by the bite of a rabid animal or from the scratch by the claws of a rabid animal which has previously licked its claws.

Bites upon the exposed parts, as the hands and face, are more fatal than those through the clothing, as the clothing tends to clean the teeth of the saliva before reaching the skin.

Spontaneous cases of rabies are now known to be impossible. It is accepted by all authorities that the virulent principle is a living germ; but, unfortunately, all attempts thus far to isolate and distinguish it have failed. This virulent principle is found in the tissues of the brain, spinal cord, saliva, and other fluids of the body. The injection of a very small part of the brain or saliva of a rabid animal produces a fatal general disorder. Inoculation from one animal to the other may be carried on indefinitely. This, together with the fact that the period of incubation is more or less prolonged, would tend to show conclusively that rabies is not the result of poisoning by some chemical agent, but that it is due to a living germ that propagates when injected into an animal. Rabies agrees with all other germ diseases in that it develops after inoculation, and that the attack protects usually against the second. At first it is confined to the region of the bite; but, when fully developed, it is found in all vascular tissues.

Period of incubation varies from a few days to months, the average period in dogs being about twenty-five days. In man the period of incubation is extremely variable. In 6 per cent. of the cases, it is between three and eighteen days; 64 per cent., between eighteen and sixty-four days; 34 per cent., exceeds sixty days. From fourteen days to two or three months is the limit.

Symptoms in Dogs.

For the first two or three days the symptoms are similar in all forms of rabies. There is noticed considerable change in the habits of the animal, restlessness or sullenness. In other cases, the animal

becomes more affectionate, and liable to infect by licking the hands and face of persons handling it. There may be dulness, but more frequently hyperæsthesia. The dog starts quickly at the slightest noise; has a tendency to pick up and swallow foreign bodies, as bits of wood, stone, dirt, cloth, etc. Sexual excitement is marked; sudden passion is developed when another dog is seen. The animal secludes itself in dark places. It barks and howls, and starts nervously without any apparent cause.

The bark is quite characteristic, in that it is a low, hoarse, muffled sound, beginning as a bark and prolonged into a howl, which may be repeated two or three times without closing the mouth. Following these primary symptoms, the case generally develops into one of two forms, — the furious (also called mad form) or the paralytic (also called dumb form).

The furious form is shown by paroxysms of violence, during which the animal generally escapes and runs away for many miles, attacking and biting anything in its way. It seldom stops for any long fight with another dog, but makes a quick bite, and is off again. It may or may not return home after the paroxysm has passed off. Between the paroxysms, the dog shows symptoms similar to those during the first two or three days. The paroxysms are repeated, and in two or three days there is loss of power of the hind legs, which is the beginning of general paralysis. This is followed by complete paralysis of the hind legs, which results in the animal being unable to move about. This is followed by partial and then by complete paralysis of the front legs. Dropping of the jaw, paralysis of the pharynx, and frothing from the mouth follow. Gradually complete paralysis sets in all over the body, and death results, generally within ten days of the first symptoms.

In the paralytic or dumb form of rabies the paralysis follows the primary symptoms at once, and is similar to the later stages of the furious form.

Diagnosis.

In dogs the diagnosis is attended with more or less difficulty. One is seldom called until the later stages, often not until the animal has been destroyed. Street cases are attended by all sorts of wild

stories; and it is almost impossible to obtain a clear history, especially of a previous bite. With the symptoms as described and a history of bite, the diagnosis is fairly clear. The statements of the owner or person in charge are often very misleading. For instance, the dropping of the jaw is invariably described by the owner as due to a fish-bone, chicken-bone, or some other foreign body in the throat. It is exceedingly difficult to make the owner change his mind in regard to this; and so firmly is his statement adhered to that it often results in the veterinarian making an examination of the mouth, and thus running the risk of a bite or scratch from the teeth of the animal on the hand.

An absolute clinical diagnosis cannot be made; but often very suspicious symptoms are shown, which justify further examination.

Post-mortem examination shows absolutely no characteristic lesions either to the naked eye or the microscope. Generally, the stomach contains foreign bodies; and these foreign bodies, in the absence of pathological changes, may be looked upon as pointing strongly to rabies.

The only method of absolute diagnosis is by injecting a small portion of the brain or cord of a suspected animal into rabbits, and is done as follows: The brain and upper portion of the spinal cord of the suspected animal are removed as aseptically as possible. About one gram of this is crushed in a mortar, about 10 c.c. of sterilized water is added, and an emulsion made. This is thoroughly mixed, and filtered through a sterilized paper filter; and $\frac{1}{2}$ c.c. of the filtered emulsion is injected into the subdural space. The rabbit is etherized, and a longitudinal incision made in the skin of the forehead just back of the eye. The periosteum is reflected. A trephine is introduced, and a small piece of bone is removed, and the dura exposed. The dura is punctured by the syringe needle, and the emulsion thus injected into the subdural space. The rabbit soon recovers from the ether, and eventually goes to eating, having suffered nothing from the operation.

The first symptom noticed in the rabbit is hyperæsthesia, the slightest touch causing it to start quickly. Following this there is paralysis of the hind legs, extending to the fore legs, and finally complete paralysis, followed by death. The period of incubation

in rabbits varies considerably, but the average time of death is about the fifteenth day. In the first rabbit inoculated, death may come on in thirteen days or may be delayed as long as three months.

Post-mortem examination on the rabbit also is very unsatisfactory. There is nothing to see, and the chief value of an examination seems to be to eliminate all other causes of death.

This outlines briefly the symptoms shown and the only method of diagnosis of rabies known at the present time. Rabies has been comparatively rare in Boston, but a sufficient number of cases has occurred within the last two years to demonstrate that very prompt and stringent measures should be resorted to at once. In 1896 two dogs were reported to the Board of Health as suffering from rabies. One of these on examination proved not to be a case. The other suspected case developed about December 15; and the dog was led to one of the veterinary hospitals of this city, where it died in about twelve hours. I am unable to say whether a diagnosis was made at that time or not; but a rabbit was inoculated from the dog at the Harvard Medical school, and a positive diagnosis of rabies made. The case was reported to the Board of Health on December 31, sixteen days having elapsed without any measures being taken toward preventing the spread of the disease or for the protection of the public.

Jan. 1, 1897, an investigation was begun by calling on the owner of the dog; and, to our surprise, it was found that he was also the owner of two other dogs both of which had been constantly with the rabid dog. On examination of these two dogs at this time, one was found to show very marked symptoms of rabies, and the other presented no symptoms. It was advised that both dogs be sent at once to the hospital as a protection to the occupants of the house and to prevent a possible escape of the dogs into the street. These dogs were followed to the hospital, and a quarantine placed upon them there. The first of the two dogs died at the hospital on January 5 with marked symptoms of furious rabies. The second, although in constant contact with the first dog until quarantine was established, showed no symptoms; and on January 9 the owner, desiring to prevent further expense, was allowed to take the dog home, to be there placed in safe quarantine for ninety days. This quarantine was

faithfully observed; and at the end of ninety days the dog, having shown no symptoms, was released. As late as June 30 an examination was made of this dog, and it was found in good condition.

At the time of the investigation on January 1 it was learned that the first of these three dogs, while being led through Boylston Street some time during the first of December, was pounced upon and bitten by a St. Bernard dog; and thus there was a clear history of bite. No trace of this St. Bernard dog was ever found; but it is probable that it ran off, and died somewhere within the usual time of ten days, long before the case of the dog bitten by it was reported to the Board of Health.

Here is a good example, showing the urgent need of reporting early such suspicious cases. Thirty days elapsed from the time of the bite and fifteen days from the time of death before the proper authorities had any knowledge of either,— ample time to have caused a wide-spread epidemic of rabies in this city; for at the same time there were two more dogs at the owner's premises, one of which had rabies, and was liable to escape at any time. Fortunately, these dogs were of very fine breed, and consequently were never allowed off the premises of the owner except on a leash.

In this case, all feeding-pans, dog-bedding, dog-boxes, etc., used were destroyed, and the room washed with corrosive sublimate.

Jan. 11, 1897, two more dogs were reported rabid in the same district. Although it was impossible to connect these cases directly with the St. Bernard dog, it was possible to exclude the last three dogs as causes. If these two cases were caused by the St. Bernard dog, it is probable that they were not bitten later than December 5; and there would then have elapsed thirty-six days from the time of bite by the St. Bernard dog. The owners of these two dogs were advised to kill at once, which was done.

During the summer of 1897 a number of dogs were bitten by a St. Bernard dog in Dorchester. This St. Bernard dog disappeared, and no trace of it has been found. All dogs known to have been bitten by this dog were killed by the owners or died at different hospitals.

On Dec. 1, 1898, a dog owned in West Roxbury was reported to the Board of Health as a case of rabies by the attending veterinarian,

who also killed the dog. The head of the dog was obtained, and sent to the Board of Health laboratory for examination. This was the first case which occurred after this laboratory was available for the purpose of examination and diagnosis of suspected cases of rabies.

A rabbit was inoculated in the usual way by Dr. Hill and myself on December 5. The rabbit is alive and well to-day, and has shown no symptoms of rabies. The dog was known to have been bitten by an unknown dog on September 15; but no symptoms were shown until Nov. 27, 1898.

There has been recently in Boston an outbreak of rabies which derives considerable interest from the following points: —

No history of the original dog having been bitten could be obtained. The affected dog had short repeated paroxysms, during which it would attack everything in its way. These paroxysms lasted for so short a time that the dog did not leave the district, but returned home after each. A great many dogs were either bitten or exposed to infection from this dog. A small boy was also bitten by the dog during one of its paroxysms. The danger from this outbreak was looked upon very lightly by a majority of the dog-owners. The conditions of quarantine maintained on the owners' premises were such that, while the quarantine protected the public to a certain degree, it afforded no protection to the owner or any one else who might visit the premises.

It is doubtful if this case would have come to the knowledge of the Board of Health, except for the fact that a boy had been bitten by the rabid dog. Saturday, Dec. 3, 1898, this boy, about twelve years of age, was bitten on the hand by this dog, and a slight wound inflicted. The owner of the dog joined the parent of the child in taking the child to Dr. Green, of Dorchester, who dressed the wound. Dr. Green requested the owner not to kill the dog, but to report to him at once if it should show any suspicious symptoms. On the following Wednesday, December 7, the owner reported to Dr. Green that the dog was acting in a suspicious manner, and was advised to have a veterinarian see the dog at once. The veterinarian, after hearing the statement of the owner, reported the case to the Board of Health immediately. I examined the dog at once at the owner's

house, and found it paralyzed on the kitchen floor, biting and snapping at everything within reach. It was immediately placed in a large box, chained up, and boarded in to prevent any handling by the occupants of the house. As was expected, the dog was found dead the following morning. A second dog owned by the same person, and known to have been bitten by this dog, and at the time of the first examination found playing with a small child in the house, was at once killed. A great many conflicting stories were obtained at this time from the owners and outsiders, the owners being more suspicious of rabies than the outsiders, who made light of the whole affair. A diagnosis of rabies was made, and the case treated as such.

Here was a case that called for prompt action. A child had been bitten, and it would have been criminal to neglect it. On consultation with Dr. Green the parent of the child was informed of the possible danger, and advised to send the child to the New York Pasteur Institute for treatment. This was done within a week.

All other dogs in this vicinity known to have been bitten or exposed to this rabid dog were placed in quarantine upon the owners' premises. All of the owners of suspicious dogs acquiesced in this quarantine, and made a very easy matter of it.

Although this quarantine on the owners' premises has been well observed, it was seen early in the outbreak that at the best it was a very poor method of quarantine; and, as a result, the Board of Health at once directed the erection of a detention hospital for dogs, to which all dogs not properly quarantined could be removed, and where their safe isolation would be insured.

Just a word in regard to this hospital. With the consent of the Election Department, a wooden polling booth was transferred to the Epidemic Hospital grounds on Swett Street, and a number of kennels at once put up in it. In the rear of each kennel is an independent yard. The partitions between the yards are of heavy wire, and all separated from each other by a four-inch space, thus preventing the possibility of dogs reaching each other through the meshwork.

On December 9 an autopsy was made by Dr. Hill and myself at the laboratory on the rabid dog, at which no pathological changes

were found. In the stomach was found a large mass of cloth, which, in the absence of pathological changes, was enough to warrant suspicions of rabies. A small portion of the brain and cord of the dog was injected into the subdural space of a rabbit in the usual way. On the eleventh day the first symptom, that of screeching, was observed, together with partial paralysis of the hind legs. On the twelfth day distinct attempts at screeching were noted; but the attempts were without success, probably due to the fact that there was paralysis of the larynx. Paralysis had extended to all four legs. Death resulted on the thirteenth day. An autopsy made at once showed no pathological lesions.

One of the dogs bitten by this one became rabid, and was killed by the owner. Seven other dogs were at once killed as a precaution also by the owners, and sixteen are now in quarantine. This outbreak has demonstrated to this department the necessity for such action as the following recommendations outline: —

1. There should be an improvement in reporting cases, for it is believed that many never reach the proper authorities.

2. The early report of all suspected cases. I think there is a tendency on the part of some to hesitate in reporting these cases for fear of an error in diagnosis. It hardly seems as though there could be any harm come by reporting these cases early, no matter how slight they may be.

3. The compulsory reporting by police, owners, and veterinarians of all dogs killed by them as dangerous animals. And in connection with this case I will just call your attention to the Melrose case, which you probably have all heard about, that of a dog which ran into a church on November 14, and bit a woman. That dog was killed within a few days, I don't know but it was the same day, by some one at the Melrose railroad station. Nothing was done or said about the dog at the time. It was not considered or thought of as a case of hydrophobia. The latter part of December or the first of January the woman died with hydrophobia. I mention this case to show that there was a possibility of treatment for that woman, if the case had been reported at the time the dog was killed. It was killed probably within twenty-four hours. A diagnosis could have been made then, within fifteen days, which would have been

before the last of November,— ample time for the woman to have gone on to New York for treatment, which would then have given her almost a month for treatment.

4. An autopsy should be made upon all dogs killed, if the symptoms are at all suspicious, particularly dogs killed on the street, the history in the latter cases being usually very unreliable.

5. The inoculation of rabbits for diagnosis from all cases reported to the Board of Health as rabid.

6. The strict quarantine for ninety days of all dogs bitten or exposed to infection from a rabid dog.

I think in this case the owners should be given the choice either of killing or quarantining.

The quarantine of the dog on the owner's premises should amount to more than preventing its escape. It should also insure the safety of the occupants of the house as well.

7. A detention hospital under the directions of the Board of Health, to which all dogs not properly quarantined at home may be removed. It would be convenient in connection with this if we had the authority to compel the removal and quarantine at such a hospital of all dogs which, upon investigation, had been exposed to infection.

8. All utensils used by the dog should be disinfected or burned, if necessary. The dog should be cremated. The walls and floors of the rooms in which it has been quarantined should be washed with corrosive sublimate, with a strength of about one to five hundred, or 5 per cent. carbolic acid solution. And in connection with this I would say that all corrosive sublimate solutions should be acidified or contain a small portion of common salt to prevent a precipitation of albumen: bichloride, 1; salt, 5; water, 1,000. Bichloride, 1; acid, 10; water, 1,000.

The Board of Health is now prepared to make inoculations on rabbits from all dogs suspected of rabies and to remove all such dogs to its detention hospital. It is also hoped that, with the improved laboratory facilities now at hand, it may do considerable experimental work in connection with the disease.

THE CHAIRMAN.— Any remarks to be made upon Dr. Burr's paper?

DR. ABBOTT.— I should like to inquire among those regulations whether there is no regulation in regard to muzzling dogs.

DR. BURR.— Mr. Chairman, I purposely avoided going into that side of it. It had seemed to us that the muzzling of dogs is very unsatisfactory. The muzzle had to be removed from the dog at least twice a day for feeding, which of course was a risk, and also it is very frequent to find dogs in the street with muzzles off the head, hanging loose about the neck. While I think that in some cases there may be considerable benefit from the muzzling, a great many times it is of little value. While I do not recommend muzzling dogs all the time, as is done in some countries, there is no doubt but that, if all dogs were constantly muzzled in a proper way, rabies would be practically an unknown disease.

DR. MILLER.— Mr. President, I was very much interested in this paper,— an exceedingly scientific paper. I watched the doctor closely to see if he was not going to give us some remedies, some treatment. I really hope that he will be able to, or some gentleman present will. The only points I saw in that direction were three,— one was to quarantine the patient until it died, the other would be to kill the patient outright, and the third would be to send it to New York. I would like to know if the doctor can give us any remedy that we can apply immediately, or if any physician present can, some course of treatment which we can apply to our patients,— a course which we can have the patient follow at home.

DR. BURR.— In regard to the treatment of rabid dogs, that, so far as I know, is not attempted. The treatment of the human patient has been carried on at many anti-rabic institutions with considerable success.

DR. MILLER.— The point I wanted to get at, Doctor, was whether or not there was any remedy that you could suggest that we could apply here,— that physicians could use, and keep the patient at home,— or if it would be better to send him immediately to the Pasteur Institute.

DR. BURR.— So far as I know, there is no treatment for a human patient in this State, that is, there is no anti-rabic institution in this State.

DR. ABBOTT.— Mr. Chairman, I should like to say one word

about this matter of muzzling. Muzzling dogs may not be an agreeable measure, in its practical application; but how is it in other countries? I once listened to a discussion in London on that subject. The question came up, and was introduced in the Congress of Hygiene, Shall Dogs be Muzzled? There was a vigorous opposition at once. The hunting privileges of the noblemen must not be interfered with. On the other hand, how is it in Berlin? No dogs without muzzles: no hydrophobia. Every man, woman, and child vaccinated at the age of one or two years and again at twelve years: no small-pox.* [Applause.]

A MEMBER.— Mr. President.

THE CHAIRMAN.— Dr. Gage, I believe, had the floor previously.

DR. GAGE.— I should like to say in beginning that, if I remember rightly, at one of the very first meetings of this Association we had two very interesting papers on this same question by Dr. Ernst and Dr. Peters. As I remember, one statement of Dr. Peters was that this disease suddenly appeared, and then by extension spread in the neighboring towns, and then disappeared, and after a certain period appeared again. I wondered if any observations had been made to determine any laws that governed that spread or the period at which it appeared, whether anything could be done in the way of forestalling any possible dangers from it, as, for instance, if the disease appeared in one town, whether notification might be sent to neighboring towns and to neighboring parts of the State, or any precautions taken. I will also put another question before I sit down, and leave the answers of both to come at once; and that is, the practical difficulties that the doctor has to contend with. A boy is bitten by a dog. I have two cases in mind that happened within a couple of years in my own practice. A man, intelligent, came to me, and said: "My boy has been bitten by a dog. They think the dog was mad.

* Examination of the records of England and Germany for the four years 1892–95, reveals the following facts: During these four years forty-three persons died in England of hydrophobia in a population, in round numbers, of thirty millions. During the same years, in Germany, in a population of fifty millions, only eight persons died of the same disease; and all of these eight cases without exception occurred in Königsberg, Silesia, Breslau, and other districts near the Russian and Austrian border, into which vagrant and rabid dogs may have strayed across the border from countries where dogs were not under strict control. So also with small-pox. Nearly all of the very few deaths from small-pox in Germany in recent years have occurred among immigrants from neighboring countries where vaccination is loosely conducted or is not compulsory.

They have killed the dog, and buried him out here in the country somewhere,"— he did not know exactly where, thought he could find out. "What would you do with the boy?" I did not know how long after death the dog's brain could be utilized for the experiment with a rabbit, but in a general way told him that, if possible, I should get that dog's head and have it sent to Boston at once, and then, if they found rabies, send the boy to New York. Unfortunately, it was not done; and the boy died with rabies. One of the selectmen of a neighboring town came to me, and said: "Doctor, what shall we do? A dog has bitten two boys, and we don't know whether the dog is mad or not." I believe in that case they lost track of the dog. The question that I had to answer was, Ought I to send those boys to the Pasteur Institute on the chances? I should like to say right here that my observation is that still a great many men in the profession take out their nitrate of silver, and send the family home comforted. I don't believe in that. I think the profession, the medical profession, would like to have the veterinarians lay down some rules which we can go by in giving our advice to our patients. Have I made my questions clear enough, so that the doctor can answer them?

DR. BURR.— It seems to me, Mr. Chairman, that those questions are exceedingly hard to answer. It seems to me that the first attempt should always be to locate the dog; find the dog, if possible, and submit the dog to an examination,— that is, by an inoculation upon rabbits,— and in that way confirm the suspicion.

DR. GAGE.— After the dog has been killed, how long will its brain be serviceable if it has been buried?

DR. BURR.— That would depend, it seems to me, entirely on the condition of the brain as we received it at the laboratory. If there was marked decomposition of the brain, I think it would be hardly worth while attempting to make the examination. At this time of year, of course, the head might keep in very good condition, and how long it would be impossible to say. If at once placed in an ice-box and frozen, I suppose it would keep for some time. I remember a paper by Dr. Frothingham, in which he spoke of the time that the emulsion might be kept after it was made from the brain. He had used an emulsion for injecting as old as a year and ten months, I

believe, which had been frozen at a very low temperature all this time, possibly at a lower than 32°.

Dr. Gage.—If the cases were suspicious, but you had not a positive diagnosis of the brain, if the presumptive evidence was in favor of rabies from the history that you could get, would you recommend that we should tell our patients to go to New York?

Dr. Burr.—A case that occurred in Boston was somewhat similar. The dog, while presenting symptoms of rabies, was almost in the last stages of it; that is, it died probably within twelve hours from the time it was seen. There was no history whatever of bite in this dog. I tried the best I knew how to trace it back, but it was impossible. The dog was presenting some symptoms similar to rabies, and a child had been bitten. In consultation with the parent of the child and the attending physician, it was thought best to let the parent know, and state to him just the dangers of the case; that his child had been bitten, and, while we could not say positively that it was a rabid dog, it was impossible to determine for fifteen days, and it did not seem as though it was wise to wait for fifteen days. With this knowledge the parent was to decide for himself. In his case, although a man working for $2 a day, he decided to go to New York at once; and I have no doubt that the total expense was about $300.

Dr. Parker.—Mr. President and gentlemen, the great trouble that the Cattle Commissioners have found in locating cases of rabies is due to the difficulty in getting the owners to report any such cases. No one ever reports until a number of dogs have been bitten. Another trouble, even after positive evidence of rabies has been found, is the difficulty in getting the local authorities to order the muzzling of dogs.

An interesting case occurred in Ipswich a little while ago that illustrates this point. A dog on the sixth day of December went rabid. It bit several other dogs, and among the dogs which were bitten was one belonging to a Mr. Kinsman. This dog was traced to the woods round Hamilton, and finally was shot in the woods and left there. Nothing was done about it, and no report was made of it until on December 21, just about a fortnight after he was bit, the dog belonging to Mr. Kinsman went rabid. It was not till then that the selectmen of the town took any action. They did not report the

case, but they tried to get the owners of dogs in the district to muzzle the dogs; and there the trouble began. The minister of the place was among the owners, and he absolutely refused to properly muzzle his dog. He said that his dog was muzzled if he had a muzzle tied round his neck, and that was all he was going to muzzle his dog, anyway. He said that Ben Butler had won a case in court on that very point. At that time the order read that a dog was to "wear a muzzle." Ben Butler won his case on the wording of the order, that the dog was to "wear a muzzle," the court holding that a dog was "wearing a muzzle" when it was hanging round his neck. Since then, of course, the wording has been changed. But the minister absolutely refused to properly muzzle his dog.

Last fall several cases of rabies occurred in the neighborhood of Lynn, Swampscott, and Salem. The chairman of the board and myself went down there; and we noticed — it was after a muzzling order had been issued — that a large portion of the dogs in the street were wearing the muzzles round the neck. They were not properly muzzled. The difficulty in carrying that order out is in consequence of the opposition of different people to muzzling their dogs. They seem to think it is a cruelty. It may be so in isolated cases; but, if the dog is properly cared for, and using proper precautions when the muzzle is taken off at home, I think that there need be no trouble at all about the muzzling of dogs. The muzzle is specially intended to be worn when the dog is on the street or at large.

This case at Ipswich that I referred to is a characteristic one, and rather an interesting one. This second dog, that went rabid on the twenty-first day of December, was found by his owner, Kinsman, in the barn in the morning, snarling and biting at his cows. He drove the dog off with a club. The dog went out of the barn, and bit two or three of the neighbors' dogs; and, finally, that day he was shot. There was no report made about it. The proper authorities did not know about it at all until, on the twelfth day of January, one of the cows went rabid. The cow was found in a paroxysm in the morning, was taken out from among the other cows, moved to a loose box, and there she burst the hinges off one of the doors, she broke three or four boards off the box-stall, she bit at the wood so that it showed the marks of her teeth, and she threw herself on

the ground and beat her head on the floor so as to knock the horns off and reduce the core of the horn to pulp. Finally, she reared up on her hind legs, and fell down and died. The cow was then hauled out and laid in the snow over night; and that probably helped to preserve the brain, which was taken out the following day and sent to Boston for inoculation. No results have yet developed from that case.

In considering the subject of treatment, the board came to the conclusion that, in consequence of the exorbitant charges at the Pasteur Institute, it would be a good thing if the State, either the State Board of Health or some State institution, should treat parties that had been bitten by rabid dogs in Boston. I think it would be a very good thing: it would not necessitate the cost of conducting a separate institution; and it seems to me that something like that ought to be done. There have been, I think, thirty-three cases of rabies throughout the State during the past year, in dogs and other animals. There was one case in which a horse developed rabies. A year ago there were several cases in cows which had been bitten by dogs. Although it is not increasing, yet it is certainly not being reduced. One of the principal difficulties is, as I say, the difficulty in getting people to muzzle their dogs and in properly enforcing the licensing of dogs. I think this is a very important thing. If the local authorities would enforce, strictly enforce, the licensing of dogs, it would greatly help to reduce the danger from the curs and stray dogs that run about almost every village, it would tend to reduce the danger from that class of animals.

Dr. H. G. Stetson.— I should like to ask Dr. Burr if he knows of any place in the country, besides the Pasteur Institute in New York, and one place in Baltimore, where treatment can be carried out for the prevention of an attack of rabies. I know that the charges at the Pasteur Institute are very large; and, as Dr. Burr says, to a man earning only $2 a day, it seems an enormous expense to assume merely upon supposition. I have often wondered if there was any place in the country, aside from those two places, where patients could be sent at a less expense. I don't know exactly what the charges are at the Pasteur Institute. Those

at Baltimore, I believe, are $150; and in addition to that there is, of course, the expense of keeping the patient for three or four weeks,— that is, his board and general living expenses while he is undergoing the treatment. Some four or five years ago I was in Baltimore at the time that Dr. Kyrle and Dr. William H. Welsh of Johns Hopkins were conducting their experiments in rabies and at the time that they were preparing to inaugurate their treatment for rabies; and I saw very many cases of rabies from inoculation experiments in rabbits. Those cases were pretty indelibly fixed upon my mind, so that it will be a long time before I forget them. Dr. Burr says one thing which I should most heartily concur in; and that is, by all means make people understand that after a dog has bitten a person they never should kill the dog. The dog should be allowed to live, and live under the closest observation. In that, in a measure, rests the treatment of the bite upon the person.

DR. COOK.— I should like to ask the doctor, In how short a time after a dog has been bitten by a dog is it possible to find the virus in the saliva or in the mouth, and is it possible to find the virus there before he produces symptoms of rabies?

DR. BURR.— I think the period of incubation varies considerably in animals. It may come on in ten days. Twenty-five days is perhaps to be looked upon as an average time. I think that certain cases have shown that the virus may be present in the saliva some time previous to the animal presenting symptoms,— how long I really can't say. In regard to the question of the previous gentleman, as to the places for treatment, I don't know of any place outside the places which he has spoken of.

A MEMBER.— I should like to ask the Doctor if he has noticed any particular characteristic, any particular kind, class, or condition of dogs that are predisposed to rabies, or whether there have been any and all kinds that have come under his care and consideration.

DR. BURR.— It is common to all dogs. Breed or class has nothing to do with this disease.

A MEMBER.— I should like to ask Dr. Burr whether he thinks the treatment at the Pasteur Institute can be recommended as absolutely harmless, whether any cases of rabies result from treatment, whether any suspected cases may go there as the result of

treatment, and also whether he goes outside of the city of Boston to investigate suspected cases.

DR. BURR.— In regard to the last question, I can answer that very readily. We have no authority outside of the city of Boston. In regard to the first question the harm, if any, must be slight. I have never heard of any cases of rabies resulting from Pasteur treatment.

DR. SHEA.— I should like to ask Dr. Burr if the treatment at the Pasteur Institute is a specific one. Is it really in the same line with the serum treatment of diphtheria? Have there been any cases sent to the Pasteur Institute that have died with hydrophobia?

DR. BURR.— If there are any cases which died, which have been sent there?

DR. SHEA.— Yes, after having the treatment.

DR. BURR.— I don't know what the percentage of death is of cases treated at the Pasteur Institute, I am sure. I know that all cases are not successful, by any means; but it should be said that the treatment is not expected to be successful, if begun after symptoms of rabies have set in. In regard to the treatment, it is not on the same lines at all as the serum anti-toxine of diphtheria. The treatment consists in the injection of very dilute attenuated virus, and gradually working up to a virus equal to the normal strength of virus from rabid dogs. To go into it in detail, the rabbits are injected from rabid animals, and probably die within thirteen or fifteen days. Rabbits inoculated from this rabbit die in a shorter time. This is repeated from one to another until death occurs in about six days. That is probably the height of the virulence in rabbits. The cord of a rabbit dying within six days is removed, and divided into parts and dried at different periods, I believe, from one to thirteen days. While drying, it seems to lose some of its virulence; that is, dried for one day, there is a certain loss of virulence; two days, a certain loss, and so on. Cords dried thirteen days are comparatively weak. Injections are made subcutaneously into patients of an emulsion of cords dried for different periods, working up from the thirteenth day to the first day or cord dried for one day only. The treatment takes about two weeks. In that way the patient, as

I understand it, is able to stand the injection of a virus of about the same strength as the virus of the dog from which the patient had been bitten.

THE CHAIRMAN.— It strikes me, gentlemen, that Dr. Gage has asked a most practical question. Whether everybody feels satisfied as to what rules should be our guide, I am not able to say; but it strikes me as a matter in which every physician must be greatly interested. A child has been bitten, and brought to the doctor for advice. The question is asked, What shall be done with this child, immediately and afterward? Should this child be sent to the Pasteur Institute or some other place where treatment may be given, or not?

It is a very important question. And there is another question, which may have priority; and that is, What shall be done with the suspected dog which may be found dead? It strikes me that we can come to but one conclusion. If the dog is alive, he should be confined and watched until there is some satisfactory result as a guide. If the dog has been killed, find him; and, unless he should be found in a very advanced state of decomposition, his brain should be secured for examination. In the absence of finding the dog, there is a duty for some responsible person. Let the physician determine as to the probability of the dog being rabid; and, if the probability is strong, he should advise the parent to send the child for treatment. How can we do less, and do our duty to the parent, friend, or the patient himself?

Another question which struck me as very pertinent, indeed, in this line was concerning the trustworthiness and expenses of the different institutions where treatment is given. Perhaps Dr. Burr would be willing to ascertain at least the expenses of these different institutions, how many there are and where they are, and report to the Association as much information concerning them as may be practicable and pleasurable to him. He promises it.

Are there any other remarks to be made?

DR. GAGE.— May I ask just one question?

DR. DORMAN.— I should like to ask if there is any harm to come from the treatment.

DR. BURR.— I think I should rather refer that question to Dr.

Parker, who, I know, has been there for treatment. Perhaps he can give you some information on that point. I don't know of any harm that can come. I know Dr. Parker has told his experiences, and perhaps he would relate those again to you.

THE CHAIRMAN. The question is, Is there any one present who can state that any harmful effects have been known to arise from the treatment, such as the Pasteur method of treatment?

DR. PARKER.— Mr. Chairman and gentlemen, so far as I know, there is no harm from the treatment; but there is a good deal of discomfort. I was down there, I think, about six years ago, and underwent the treatment for a bite from a rabid dog. The treatment was somewhat like the following. You went down there to the institute and there injections of the solution were made into each side. That was repeated in the evening again, and every following day for some days; and then it was done just once a day. You were not detained at the institution. You were allowed to take the rest of the time to yourself; but you were expected to go there at first twice a day, and then later once a day. The discomfort arose from the pain from the injections into the soft muscles. After being continued for several days, it got very painful. I can easily imagine how a child especially would seriously object to it. But, so far as any bad effects were concerned, I don't think there were any. When I first went down, I had to have the horse-car absolutely stopped when I got off at the institute, because every time I stepped off the car it would jar me from the top of my head down to the soles of my feet. That got worse for two or three days at first, and then gradually disappeared. But the discomfort principally arose from the soreness at the various points of injection. I believe there is no case on record where any person has come down with hydrophobia after the course of treatment there, unless he went too late. There are several cases where people have died from it, when treatment began after they had shown some symptoms of nervous disturbance; but I understand — and I think that Dr. Winchester is of the same opinion, he has been through the treatment, too — that their records show that no cases of hydrophobia have developed after the treatment, if begun in time.

DR. GAGE.— There is one question I should like to ask Dr. Burr. It is the question that the doctor is asked by the family, and I should

like to ask him to answer it for the Association. They say, Doctor, supposing this child of mine has been bitten by a rabid dog, how soon must he go down there in order to be safely treated; that is, what is the longest interval during which treatment can be safely delayed? That is one question we have to answer.

DR. BURR.— I am under the impression that the Pasteur Institute is in the habit of telling patients that, if they come later than about ten days, they do not guarantee a cure. In the case of the boy that was bitten: he was bitten on a Saturday, and went to New York on the second following Sunday night, which was about eight days. I suppose he arrived at the institute on Monday. Those in charge at the institute told the father that, if he had allowed it to run two days longer, they would not guarantee a cure.

DR. PARKER.— Mr. President, if I might again take up the time for a second with reference to the Pasteur Institute in New York, my reason for advocating some method of treatment in this State would be simply the cost of the treatment. The principle of treatment is right, but the cost is excessive, especially for a poor man; and, if the State is going to undertake the preparation of anti-tetanus serum and serum for the treatment of diphtheria, it would seem as if something might be done in this other direction as well.

DR. SMITH.— I suppose that the members of this Association do not realize to what extent the anti-rabic treatment is being applied throughout the world. I think the statistics probably go into the hundred thousand cases treated up to date. There were some twenty-four anti-rabic institutes in the world in 1894. Almost every State in Europe has one; and, as you know, there are several in this country. So far as the statistics go,— and I have looked into the subject recently quite carefully,— there appears to be no danger from the treatment; and it seems to me that the advice that a physician should give, if there is any doubt whatever, is for the patient to undergo the treatment. That will relieve the physician of all responsibility later on.

As regards the expense, it must be borne in mind that every institution is under the same expenses, whether it treats two patients or one hundred patients, with the slight increase of *personnel* to make the injections and do the clerical work. It is absolutely nec-

essary to keep up the series of inoculated rabbits. It is necessary to take out the spinal cords of these rabbits at such and such a time, and prepare them for the injections. If they are dried longer than, say, thirteen or fourteen days, they are of no use, so that it is necessary to keep up the main work of the institute if there is not a single patient there. The rabbits must be inoculated at proper intervals; and, if there are no patients, the material would simply have to be thrown away. This should be borne in mind in criticising the excessive expenditures of these institutions, because there must be a plant with so many rooms for the preparation of this material, whether there are thirty cases in the year or a few thousand.

In regard to the appearance of the disease after treatment, it should be stated that the European statistics show that now and then the original disease breaks out during treatment and sometimes fifteen days after. It is generally assumed now that it takes at least twenty days after the beginning of the treatment for full immunity to be produced; and, if the disease of the patient is going to appear clinically during those twenty days, of course the treatment has no effect, so that the disease now and then, after severe bites on head and face, may be expected to occur a few days after the treatment is over. But, so far as the statistics go,— and I believe that most of the anti-rabic institutes are managed by men who furnished entirely reliable figures,— I think there is no evidence that there is any danger from the treatment; and, if I should be bitten by a susceptible animal, I think I should at once prepare to submit myself to the treatment if there was the slightest suspicion that the animal was rabid.

Dr. Burr.— Mr. Chairman, in regard to further answering Dr. Gage's question as to the length of time that might elapse before treatment was begun after a bite,— referring to the Melrose case, a certain veterinarian of Malden was called to see a dog in Melrose November 23, a woman having been bitten in church by the dog on November 14. At that time, on close questioning, by the veterinarian, of the owner of the dog, he could obtain no history of bite; and the owner insisted that the dog had a fish-bone in its throat, as they had had fish the day before at the house. He ventured to put his

hand into the mouth to remove the supposed fish-bone, and either scratched his hand or was bitten by the dog, I couldn't say which, but thought nothing of it at the time. The dog died the following day. Nothing was thought of it then. He went off, I believe, on an outing in Maine, and while there one night had a dream that he was being bitten on the finger by a dog. I believe he had at that time a spasm: whether it was a nervous spasm or not I don't know. He returned, and consulted friends, with the result that nothing was thought of it. About January 2 report came that the woman who had been bitten on the 14th of November by a dog had died of hydrophobia. In the mean time it was learned that the dog which had bitten this woman, also bit the dog seen by the veterinarian. The veterinarian immediately went on to the Pasteur Institute January 2, which is a lapse of some fourteen odd days. I believe he has had a number of spasms there: whether they are entirely nervous or not I cannot say. He was to return a week ago last Tuesday. He did not return on that day, but I have not heard that there are any serious results. He expected to be cured, I believe. That was forty days after the bite.

THE CHAIRMAN.— Any further remarks to be made upon this paper? If not, Mr. Morse desires to say a few words concerning an incinerating plant recently erected in Boston, and which was put in operation yesterday.

THE REFUSE DISPOSAL PLANT OF BOSTON.

Colonel W. F. Morse.— Mr. Chairman and gentlemen, as a good many of you know, I am an advocate for the disposal of garbage by cremation. Five years ago we discussed the merits of reduction and cremation at one of our meetings, and came to no conclusion. Two years ago we discussed it all the afternoon at Fall River. The arguments were fairly presented on both sides; and still it was not possible to determine which method was better, except that cremation was making progress, while reduction was going backward. At the present time, seventy-five or eighty or nearly one hundred cities are using cremating furnaces for garbage and waste. Five or six at the utmost are using the reduction process, and their numbers are being reduced. The result is that cremation is progressing, and reduction is receding.

What I had to say was this. The efforts of the builders of cremating furnaces have been to utilize the heat in a way to make it more effective. The late Colonel George E. Waring brought from Europe two years ago the idea of a long travelling belt, on which could be placed all the waste, from which parts could be picked out and sold, and the remainder burned. He tried the experiment in New York. He caused to be built a furnace. He collected from two hundred thousand people all the combustible and paper waste, separate from garbage and ashes, and sorted it over. After picking it out, he found that 10 per cent. only remained to be destroyed. 90 per cent. was salable, and valuable for marketable purposes. That experiment has been tried two years. It has developed two facts: one is that the combustible refuse of a given number of people is worth saving, provided it is large enough in bulk to be handled on a large scale by machinery; the second fact is that the combustible refuse of any city will furnish heat enough to burn the organic waste of the same community.

Boston has been peculiarly fortunate in having a progressive mayor, a progressive board of health, and a progressive corps of engineers. They tackled this question last April, advertised for bids, and got five propositions, which they promptly rejected because

they were not practical. After that there came to them in an informal way a proposition which they did put into use, and which has resulted in building within six blocks of this place a Refuse Disposal Plant, in a building which is 165 feet long, 75 feet wide, and to which is brought all the combustible refuse of every character and kind from about two hundred thousand people. This building contains a long travelling endless belt of steel plates. The carts dump upon the first twenty-five feet of the belt. It travels along 100 feet further; and during the time it is passing twenty-five men on each side pick out five or six different classes or kinds of paper scraps, rags, leather, glass, iron, everything that is marketable and salable. After they have picked this over, they have removed 80 to 90 per cent. from the whole quantity. The remaining 10 per cent. goes by an inclined conveyor into a bin, and from there is put into a large furnace, which furnishes heat to move the conveyor and to light and heat the building. This plant was built at a cost of about $30,000. It was yesterday for the first time put in operation. They brought about 150 cubic yards of waste, and at noon the whole quantity was sorted and burned; and the men were waiting for more. To-day, I am informed, they will bring about 250 yards,— a pretty fair test of a plant for the second day of beginning work.

We do not know how much heat this will generate. We do know this: that, when you take a piece of paper and burn it, you get a degree of heat equivalent to a certain amount or proportion of coal which is the standard of measurement. It does not go as far as coal; but it is certain that there is enough heat which, if used in a proper way, can be utilized for the destruction of matter other than itself. I believe it to be the case that every garbage furnace in the country is throwing up its chimney at the present time waste heat which, if properly utilized, could burn the garbage itself over again. I believe also that this point has been attained: that every city or town of a population of ten thousand, with a proportional amount of garbage,— we will say, for instance, five tons per day for a population of that number, and an equivalent amount of combustible waste,— if it will bring to one point the garbage and the refuse separate, one can be made to destroy the other without cost. If a city of fifty thousand people, or any community of whatever size, will perform the same

thing in the same way, making three separations,— the garbage, the refuse, and the ashes,— it can use the worthless refuse to burn the garbage, and can sort out from the refuse itself more than enough marketable product to pay the expenses of the whole. That means that the work can be made self-supporting, and that is the object and the point that we are all after. This will presently be determined. At this time the question is in such a shape that in the course of the observation of this plant here, coupled with one or two others that are about to be built, we shall be able to state positively that with a given quantity of combustible refuse, weighing a certain amount and occupying a certain space in bulk, we can destroy another given quantity of organic refuse within a given time and at a given cost. That is the point we are after. Five years ago we had begun, two years ago we were progressing, and in a year from now I believe that almost every one of the cities in New England will utilize its combustible waste to destroy its organic matter at a cost which is practically doing it for nothing.

THE CHAIRMAN.— I want to say before adjournment, gentlemen, that there is still a trifling lack of interest in having the Association meet at different parts of the State. I don't blame you for wanting to come to Boston: that is the place to come; but you don't want to come to Boston every time. Boston does not want to appear to be selfish in this matter either. If any city in the Commonwealth wants the Association to meet there, you have only to say so; and we want you to feel at perfect liberty to give the invitation, and say when you want us to come. I won't name the cities, but there are many of them that I am afraid hesitate to give the invitation.

I don't want to say it for publication, but it has been on my mind ever since the Treasurer read his report to-day that we should congratulate ourselves on the remarkable progress we have made in the last nine years. Starting so modestly and simply as we did, with nothing in the treasury and with a small number of earnest workers in this organization, to-day we are much more than double in numbers and with something like $700 in the treasury, and with an earnestness for work such as has never at any previous time existed. Still, we never can afford to slacken our energies at all in building

up and supporting an organization like this for good work. Let us look still for larger membership, but particularly a membership of working men; and let each one suggest living topics, topics which interest the boards of health. Let every one feel an interest and a freedom to suggest questions which he wants to hear discussed at these meetings, and there can be no question as to the life and vitality of the Association doing that kind of work.

A motion to adjourn would be in order.

(Adjourned.)

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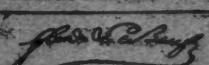

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